Essays and Addresses. By the late J. H. Bridges. With an introduction by Frederic Harrison. Pp. xxi+307. (London: Chapman and Hall, Ltd., 1907.) Price 12*s*. 6*d*. net.

THE essays included in this volume (unobtrusively edited by Prof. L. T. Hobhouse) form an admirable memorial of one of the noblest spirits that have been touched to fine issues by the "religion of humanity." It is, naturally enough, chiefly as a splendid evangelist of the Positivist movement that Dr. Bridges is considered in the introduction—itself an interesting and illuminative essay—which Mr. Frederic Harrison has contributed to the book. But there is no reader, however unsympathetic with the Comtist propaganda, who could rise from the perusal of these essays without having acquired deep admiration for the earnestness and spiritual charm, the learning, worn lightly as a flower, and presented with extraordinary vividness and freshness, the wonderful industry, fecundity, and versatility of the man whose literary achievements were the fruit of the leisure hours of a busy physician and hard-worked Government inspector.

The scientific reader who first made Dr. Bridges' acquaintance as the learned and indefatigable editor of Roger Bacon will accept almost as a matter of course the masterly summary of his long study of the great Franciscan, delivered as a university extension lecture in 1903. He will find in the oration on "Harvey and his Successors" merely another delightful example of the combination of critical, historical, and expository powers that illuminated so effectively the "Opus Majus." He will be prepared also for the familiar knowledge of the mediæval world shown in the two essays on Dante. But in these latter essays, particularly in the one entitled "Love the Principle," he will have revelation of spiritual powers perhaps unsuspected and of the noblest type. Moreover, his progress through the book will constantly deepen the impression that, even more admirable than the ability, the industry, and the taste that made Dr. Bridges so interesting and instructive a critic of topics ranging from Thales to Calderon and Diderot, was the self-sacrificing enthusiasm ever burning at the core of his indefatigable life.

LETTERS TO THE EDITOR.

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Upper Air Temperatures.

WITH reference to Dr. Chree's letter published in NATURE of April 1, may I state that the conditions I postulated *were* violated in *every one* of the cases quoted in his former letter? The ascents were all made *after* sunrise and *before* sunset. They cannot, therefore, be fairly used as evidence to suggest that my conclusions were inaccurate in the direction of underestimating the influence of radiation. At the same time, it may be worth while to consider these cases.

Dr. Chree specified three occasions on which the differences between the temperatures recorded by two instruments of different types exceeded 2° C., the maximum differences being 2°·1, 2°·5, 2°·9 C. The first, as he stated, was probably due to a scale or zero error, one thermometer being continually below the other. The third, on which he lays most stress, occurred in an ascent at Uccle on February 7, 1907. The readings of both instruments agreed during the ascent, the greatest difference being 0°·8 C. (or 1°·1 C. at the highest point). During the descent larger differences occurred. Now, at the time of observation, the sky was covered by a veil of cirro-stratus, and it appears extremely probable that the instruments, in descending from cold to warmer, perhaps saturated, air, would be affected by condensation of ice-vapour. The difference in exposure and type, combined with the bad conductivity of hoar-frost, may quite reasonably account for the differences between the temperatures of the two instruments which arose when they left the isothermal region. In addition to this, at the time of maximum difference the downward velocity was about 10 m.p.s., and there would be some lag in the instruments. This descent was, in fact, exceptional.

In the second case, an ascent at Strassburg on the same day, the readings indicate a slight lag in one instrument until the lowest temperature is reached. The sudden passage to a relatively warm upper layer was accompanied by a sudden jump of 1°·2 C. in the difference between the readings of the two instruments. The type of instrument which shows the lower temperature is the same as that which showed the higher temperature in the Uccle descent. This is just what we should expect if the instruments passed from a saturated layer, in which they became covered with hoar-frost, to a drier region. There is no record of the upper clouds at Strassburg at the time of the ascent, but it occurred simultaneously with the Uccle ascent, so that the explanation is a possible one.

In an earlier letter Dr. Chree suggested the possibility of errors of ±10° F. in the instrumental records. In order to show as fairly and clearly as possible the errors that may arise, I have taken, for Munich, all the cases from January, 1907, to March, 1908, in which the readings from two types of instrument were obtained, and the following table gives the height of the ascent, the extreme differences that occurred, and the mean of the absolute values of the differences at all the points for which they are published. The types of instrument were the same as those considered by Dr. Chree.

Height in kilometres	Extreme values of $T_1 - T_2$ (degrees C.)		Mean value of $\lvert T_1 - T_2 \rvert$
10·5	1·2°	−1·6°	0·7
10·5	0·5	−1·5	0·5
9·8	0·6	−1·3	0·5
14·8	0·6	−1·6	0·3
11·0	0·2	−0·6	0·2
12·4	2·4	−0·2	0·5
13·5	0·5	−0·2	0·2
12·7	1·8	−0·7	0·5
13·0	1·0	−0·2	0·3
17·0	3·5	−0·9	1·1
13·0	3·0	−0·6	0·9
12·9	1·3	−1·2	0·5
14·2	1·1	−1·1	0·5
13·4	1·5	−1·3	0·5
14·8	1·4	−2·8	0·8
16·0	1·7	−2·9	0·8

In interpreting these results, it ought to be borne in mind that they are chiefly from *ascents*, and include errors owing to lag, which could be largely eliminated in dealing with the observations. The records I have seen usually show that the thermometer, which is higher in the ascent, is lower during the descent, and that the lag occurs almost entirely in the worse instrument, so that the differences are representative of the absolute errors arising from this cause. Considering the very many sources of error to be guarded against, especially the difficulty of testing the instruments at very low temperatures under the conditions to which they are to be exposed, I can only regard these results as a tribute to the care and ingenuity displayed by those engaged in the experimental exploration of the upper air.

Dr. Chree does undoubted service in directing attention to the need for great care in testing and comparing instruments, but I think he is inclined to be a little unjust to those who are tackling the difficulties of upper-air investigation and nomenclature. These difficulties are exemplified by the examples he quoted and by a term which he himself accepts, apparently without demur, when he describes a phenomenon as an "inversion of temperature."

Personally, I am quite prepared to discard the term "isothermal" when another is suggested which is short, equally expressive, more accurate, and more characteristic. The greatest variation of temperature in a vertical direc-

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tion in the region under discussion is about 20° C. in 14 kilometres. Usually it is much less. In the lower part of the atmosphere the variation is generally 60° C. to 70° C. in 10 km. The upper region is therefore *comparatively* isothermal. The rapid increase of temperature between 12 km. and 13 km. in the Uccle ascent of July 25, 1907, is an example of a phenomenon which occurs in the lower layer of the isothermal region in about one-third of the ascents.

E. Gold.

Vienna, April 7.

The Greenwich Winter of 1908-9.

If the art of long-range forecasting is ever acquired, it will probably be through a careful study of past experience (of which we have now nearly seventy years' excellent *data* for Greenwich), including, among other things, what are known as "sequences," cycles (if any), and the relation of weather to solar, and possibly lunar, changes.

I propose to offer a few remarks on the state of available knowledge regarding winter, at the end of last autumn, and year, and its bearing on what followed.

By the end of November we had had only three frost days. After such autumns as the last, with all three months dry, December (as was pointed out in a table) has nearly always been mild (twelve cases out of thirteen). Thus it was pretty clear that the second half of 1908 would have less than the average of frost days (18). The actual number is 8.

The same table seemed to point to at least two of the three winter months being mild; and that is what happened, though, of course, with the cold of February the winter (proper) was slightly severe as to mean temperature and number of frost days (the latter 36, which is +3).

When the second half of a year has less than the average of frost days (18), the total, from September to May, is generally also under average (54). There are only three exceptions (1857–8, 1891–2, 1894–5) in a total of thirty-one cases. Thus it would be reasonable to expect that the total for the first half of 1909 would not be more than 46 (*i.e.* 54 − 8). There have been 42 up to the end of March, and it is possible the total may be a little in excess, conforming to the "exception" instead of the "rule" (a contingency which has often to be regarded). The number up to April 20 is 46.

If we indicate by a plus or minus sign the character (as regards frost days) of each winter (December–February) ending in a sun-spot *maximum* year, and the four winters following, we have the following table:—

Max. year	Max.	1	2	3	4
1848	+	−	+	−	−
1860	+	+	−	−	+
1870	+	+	−	−	+
1883	−	−	−	+	+
1893	+	−	+	−	−
1905	−	−	+	−	

We might here note (without pressing the point too much) that each of the first five groups has at least two *plus* values, and as the four winters 1905–8 show only one, a *plus* in the vacant place seemed the more likely. Further, one might show that the average for the first four vertical columns is a *minus* value; for the fifth, a *plus*. The winter, as stated, had an excess of three frost days.

Another point of view. We might fairly expect the next sun-spot *minimum* in 1912 (the series before is 1843, 1856, 1867, 1878, 1889, 1901). Then this year would be the third before. Consider how many frost days there were in the first half of the third year before previous *minima*. We find:—

	F.D.	Relation to average
1840	?	?
1853	42	+ 6
1864	40	+ 4
1875	42	+ 6
1886	55	+ 19
1898	21	− 15
	Av. 40	+ 4

This points to a probable excess. The number to March 31 is 42.

After a very dry autumn the winter tends to be dry. Thus of the ten driest autumns, nine were so followed. The winter (December–February) was a very dry one.

I submit, then, that, on the threshold of last winter, there was reason to expect (1) December to be mild, and frost days in the second half of 1908 under average (18); (2) at least two mild months in winter (December–February); (3) frost days in first half of 1909 not more than 46, but (4) over average (36); (5) frost days in winter, over average (33); (6) a dry winter.

While most of the above facts were, I think, known to me at the outset, I quite agree that it is one thing to make a retrospective comparison like this, and another thing to predict successfully. Of many pieces of evidence, some may seem in conflict, and one has to try and judge which is the more weighty and trustworthy. I may further admit that the cold this year has a little exceeded what I looked for.

It may be useful, nevertheless, to direct attention to these comparisons, emphasising the fact that there *is* a large body of evidence (as I believe) in relation to the character of a coming season. It seems to have been too readily assumed, hitherto, that we have absolutely no light on the subject, and that any one venturing an opinion on an approaching season is, by that fact, declared a dreamer or a charlatan, his "hits" and his ' misses" being alike mere chance.

That the winter season of 1908–9 would at least not be a very severe one (say, more than sixty frost days in September–May) I consider to have been provable by a strong consensus of facts.

Alex. B. MacDowall.

Fluorescence of Lignum Nephriticum.

With reference to Mr. Benham's letter in Nature of April 8 (p. 159), the following statements may be of interest.

The wood known as *Lignum Nephriticum* reached Spain probably about or before the middle of the sixteenth century. Monardes (1574) and Fr. Hernandez (about the same time) were familiar with the fact that a watery infusion of the wood in a short time assumes a blue colour, but they do not mention the peculiar dichroism of the infusion. This was described for the first time by Athanasius Kircher in his "Ars Magna Lucis et Umbræ" (1646), and, apparently independently of him, by Joh. Bauhin in his "Historia Plantarum Universalis" (1650).

The origin of the wood has so far remained obscure. Linnæus—and already Plukenet and Dale before him—referred it to *Moringa pterygosperma*, the horse-radish tree of India, but without reason, as was pointed out long ago. Researches, however, made at Kew within the last few weeks have convinced me that *Lignum Nephriticum* is the wood of *Eysenhardtia amorphoides*, H.B.K., a small Mexican tree or shrub of the order Leguminosæ. The blue inflorescence exhibited by an infusion of the wood of this tree is very brilliant indeed

O. Stapf.

Royal Botanic Gardens, Kew, April 16.

Morphology of the Enteropneusta.

In a paper "On the Morphology of the Excretory Organs of Metazoa: a Critical Review," recently published in the Proceedings of the American Philosophical Society (vol. xlvii., 1908), the author, Dr. T. H. Montgomery, states on p. 577, with reference to the Enteropneusta, that in Spengelia I described rudimentary pores along the whole trunk in 1899. What I did describe in this connection was a single pair of structures which I thought might represent a single pair of truncal canals and pores.

Dr. Montgomery says that he had not seen the original description, and consequently was unable to add further details. The work ("Zoological Results") can be obtained at an easy cost from the Cambridge University Press. Perhaps, however, the original description is not deemed worthy of perusal, although it would be charitable to assume that, like the proverbial egg, it must be good in parts.

Arthur Willey.

Colombo, Ceylon, March 22.

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